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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "A meridional structure of static stability and ozone vertical gradient around the tropopause in the Southern Hemisphere extratropics" *by* Y. Tomikawa and T. Yamanouchi

Anonymous Referee #1

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General comments:

This manuscript presents a data analysis connecting the ozone structure and the temperature gradients in the southern extratropics especially the Antarctic polar region. The analysis demonstrated a successful use of the ozone tropopause to bring out the seasonal evolution of the thermal structure around the polar region, including the appearance and disappearance of the tropopause inversion layer (TIL). Another significant result of the paper is a clear demonstration that the ozone heating near the tropopause does not contribute significantly to the TIL formation. Although this point is recognized in previous studies, the use of data from two stations that have different seasonal cycles in sun light exposures but showing very similar TIL structures is a very nice, conclusive piece of evidence.

In general, I find the results presented scientifically significant, and the subject is relevant to the scope of ACP. The authors did a really good job presenting their work in the context of previous studies. The current version of the manuscript, however, has several significant weaknesses that need to be addressed before the acceptance for ACP publication. Specific comments and suggestions are given below.

Weaknesses that need to be addressed:

* Sections 3.1.& 3.2 - Need to be more careful when concluding ozone structure “becomes nearly constant” poleward of 65°S in Figure 3a. As shown by the arrows, the cross section in this part of the figure is produce by interpolating stations near 70°S and the South Pole. To what extent the constant structure is due to the lack of data points in between? This data gap issue needs to be mentioned explicitly. Without further evidence, the conclusion on this can only be a weak one.

* In the last paragraph of section 3.2., the authors attribute the large meridional gradient of N2 near 60-70°S to the wind shear associated with the polar-night jet. This point is further amplified in the conclusion section (P19185L2-6). To reach this conclusion, citing a relationship as shown in Eq.(1) is not sufficient. Showing the wind field indicating the co-location and structure of the jet is necessary. The inclusion of the wind field is also important for several other statements made in the paper, e.g., L25-26 on page 19184. I suggest the authors to replace the black contours on the cross section figures by relevant wind field.

* Overall in section 3.3 too many statements are made casually without rigorous physical or mathematical arguments. Some are further included in the conclusion section. Authors are suggested to re-examine the statements there and separate those derived from the data from speculations. Some examples are discussed below.

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* Relationship between N2 gradient and ozone gradient above the tropopause. This is the most problematic part of the manuscript. The authors try to connect the structure of N2 with the vertical gradients of ozone. The statement, "Both of potential temperature and ozone mixing ratio are conserved following air parcel motions without any nonconservative processes", is a trivial one and can be used on any quantity. Although potential temperature is a quasi-conservative tracer of stratosphere, it is well known that the vertical gradient of it is not. The vertical gradient of theta, which is proportional to N2, can change without non-conservative process, compensated by the vorticity change via potential vorticity equation (e.g., see Holton 1992, p98).

In the region of weak vorticity, theta gradient term is the main variable of PV. You may consider using PV - ozone relationship to explain the similarity between the N2 and ozone variability.

Further, the statement of ozone vertical gradient and N2 are "synchronized" is not an accurate one. To be rigorous you would need to show the relationship quantitatively with correlations. If this is intended to be a casual comment and observation, you may want to consider a weaker word, e.g., "similar". This similarity, in contrast to the layer above, is an indication that the variability is largely dynamically driven. The correlation of PV and ozone near the tropopause is known. The issue of how that can be generalized to ozone gradient see comments below.

* Ozone concentration versus ozone gradient. There is a significant issue in the interpretation of Fig. 4, where the regions of low ozone gradient are attributed to the impact of the ozone hole and mixing of ozone depleted air mass in the subvortex region. Here and later the authors implicitly equate low ozone gradient with low ozone concentration. While this is plausible, the step connecting the small vertical gradients to low ozone concentration is missing.

Additional suggestions for improving the analysis:

* Contour plots with colors can sometimes be deceiving. It may enhance the message

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significantly if showing a profile plot contrast the N2 gradient and ozone gradient in the Antarctic winter.

Editorial comments and suggestions:

P19177 L6, suggest change to "... thermal tropopause can be determined by using only a temperature profile..."

L8-10, "Over the Antarctic, the temperature of the upper troposphere and lower stratosphere in austral winter and spring is extremely low. Consequently the temperature lapse rate does not change as much as that in ..."

P19178

L19-21,

"The latitudinal and vertical structures of the static stability and vertical gradient of ozone mixing ratio in the SH extratropics are captured well in this analysis."

L21, " The OBJECTIVE of this ..."

P19180 L12, "... their dynamical conditions are similar."

P19183 L16, use "Altitude" instead of "height" Similarly, in P19185, L11, and several other places, use "altitude range" instead of "height region".

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19175, 2010.

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